An Inaugural Essay on Cuticular Absorption.

Submitted to the examination of the Rev. John Mc Dowell D.D. Provost the Trustees and Medical Faculty of the University of Pennsylvania on the 12th day of April 1808, for the Degree of Doctor of Medicine.

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Introduction

Cuticular absorption, a subject which has occupied a considerable share of attention, and exercised considerable ingenuity, but which still continues to be enveloped in the deepest obscurity, has presented itself for the subject of my inaugural essay — The many interesting and useful points which this Question involves, renders it particularly important — By a satisfactory solution of it, we will be directed in the administration of many of our remedies, and the most efficacious mode of avoiding the remote and exciting causes of disease, will be pointed out. It may also afford a more satisfactory solution of that interesting physiological question, Fetal nutrition, than that at present received. Simply with a hope to raise a corner of the veil which envelopes these important subjects, and also to fulfill a law of the University, have the following experiments been instituted — In conducting them an anxious desire was felt to ascertain the truth, on which side soever it might appear — Stimulated by a theme so noble, every precaution was used to render them completely conclusive, and every source of fallacy avoided, as far as my humble talents would permit — Should they succeed in dispelling a portion of the gloom, and thereby stimulate some more skillful investigator of nature to prosecute the subject, and by completely illuminating its hol-

lowed recesses, benifit mankind; the expectations of the author will be amply gratified

An Essay on Cuticular Absorption.

A complete and satisfactory knowledge of any function, is only to be obtained by commencing the investigation at the organs primarily engaged in the performance of it. In the performance of that function which is to constitute the subject of the following pages, the absorbents are the principal agents — By the term absorbents, I mean to imply not only lymphatic vessels, but also lacteals — That the latter communicate immediately with the internal surface of the prima viæ, and are capable of taking up articles congenial to their nature, is denied by none: I shall therefore confine my present observations to the former, or lymphatic vessels, and endeavour to prove that they not only commence on the exterior surface of animate bodies; but absorb articles applied to that surface — That vegetables have vessels, commencing on their exterior surface, which absorb articles applied to it, is proved by many facts — If plants be secluded from oxygen gas, they will not vegitate, but die; as is proved by the experiments of Ingenhousz If the oxygen be not absorbed, whence its benign influence, Must we not have recourse to the doctrine of sympathy? Doctor

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Priestly's experiments unequivocally prove that carbon is absorbed by the leaves of plants, and those of Saussure demonstrate, that carbonic acid gas applied to their leaves, is essential to vegetation — abstract it, & they wither, & die — Doctor Hales, observing that the weight of plants was much increased during moist weather, rendered it probable that they absorbed water — This the genius of Bonnet left no longer questionable. He shewed that leaves continue to live for weeks when one of their surfaces is applied to water, and even nourish a whole branch with which they were connected —

The above facts prove to demonstration, that the leaves of plants not only perform the office of respiratory organs for the plant; but also furnish it with all the nourishment essential to its existance — That they are furnished with an Epidermis and true skin (those barriers to absorption) Saussure has shewn —

As we progress in the chain of creation, and approach the inferior orders of Animals; we find them all possess the function of cuticular absorption, hence if they be covered with oil, they soon die. This has been attributed entirely to the cuticular vessels being their respiratory organs. That this opinion is true so far as it comports with the vegetation of plants, I am ready to admit: But that Oxygen is the only Pabulum vitæ taken into the system by these Vessels, is by no means so obvious, and is incompatible with the follow

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"I passed a ligature (says the Doctor) round the head and tail" of a worm (The Lumbricus terrestris of Linnæus) weighed it, and placed" it in water. After remaining in it 24 hours, it had increased in weight" grains. The animal received no injury from the experiment."

Doctor Monroe in his physiology of fishes, by throwing an injection in a retrogade direction into the lymphatic vessels of the skate; has satisfactorily proved, that in that animal the absorbents do commence on the surface. The same is the fact with the Sea Egg Echinus Marinus of Linæus, and it is presumable that were not the human absorbents valvular, their evidence could readily be obtained to substantiate the above — This point being clearly demonstrated, to deny that they absorb, would be to impeach nature with a work of supererogation: For if absorption be not their office, none I presume can be assigned to them —

Having shewn that lymphatic vessels commence on the exterior surface, at least of some animals; and rendered it probable that they as well as the other lymphatics and lacteals, do absorb fluids brought in contact with their mouths; I shall now endeavour to point out the manner in which this function is performed. Of the different opinions which have been advanced on this subject, I shall adopt the following —

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The fluid to be absorbed having reached the mouths of the absorbent vessels, is by capillary attraction conveyed a certain distance into them, which is influenced by the following circumstances. The attraction of cohesion between the particles of the absorbed body, gravity, & the diameters of the absorbing vessels; these stimulated either by distention or the qualities of the fluid, take on action proportioned to the excitability of the vessels & properties of the fluid. We find therefore that fluids will be more readily absorbed in proportion to their tenuity and stimulating power. From the above position we are led to conclude, that all fluids whether they possess the principles generally considered alimentary or not, will be taken up by the absorbents, and so assimilated as to constitute congenial nutriment and become a part of the general system. That this opinion is not a mere chimera, is I think proved by the following considerations, nearly half of all the elementary bodies in nature, have been found in the vegetable kingdom. Many of them from their qualities, would appear least likely to be taken up by the absorbents; as Iron, Gold, Phosphorus, Sulphur &c. But when we revert to the fact that the Diamond, one of the hardest bodies in nature, is capable of undergoing such change as to afford vegetables their most congenial aliment, we cease to be surprised.

It is true that no principles have hitherto been detected in animals, which have not also been found in vegetables. This we ought to expect, seeing the Mammalia, the constituents of whose bodies have been principally examined, derive their aliment either directly or in-

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been found in the mineral kingdom only; but does not the existence
so many in the vegetable, furnish a strong presumption, that by a
strict analysis of the inferior classes of animals, especially the Pisces &
Insects, who derive their principal support from the former, they will
also be detected. If then the elements be all detected in animate beings;
not as extraneous agents, but constituents of the animate machine, are
we not constrained to acknowledge them Alimentaria? Elementary substances
taken into the system, if they be elements, retain their primary qualities
and properties, however they may be, by the assimilating powers of the sys-
tem, combined with other agents — That this is the case, the following facts
prove. If plants be deprived of Carbon for nourishment for a considerable
time, their usual constituent quantity of it will be diminished — If two
oaks of the same species be planted, the one in a calcareous soil,
the other not; the fibres of the former will on analysis afford the
largest quantity of calcareous earth. The same inference is deducible
from the analysis of Sea and Land plants — The former yeilding Soda
in abundance, the latter Potash — Though the evidence of the above
facts lead to the conclusion, that the constituents of plants are influ-
enced by the nutriment which they take; yet we are not to infer from
this that we are able by a particular mode of dieting them, to change
in a very considerable degree their specific properties and qualities —
The contrary is the case, as the following facts will prove. An embryo

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plant of Triticum and Nicotiana, shall be placed in the same earth, they shall both thrive vigorously, a proof that the aliment is equally congenial to their natures; yet how different their products, the former yielding food the most nutritious, the latter a substance deleterious to the human system — The Cochlearia Armoracia, or horse radish & the Sanguinaria Canadensis, or Puccoon shall be planted in the same soil, receive the same culture, and possessing roots casting out similar Radicula is it not logical to infer (admitting the doctrine of absorption which I have espoused) that they imbibe the same Materia Alimentaria? Yet how different their properties, how different their colours, though acted on by the same external agents. It therefore follows that the various modifications which the Materia Alimentaria undergo, must be dependent on the absorbent vessels and vascular system, in which the glands being principally vessels, are included — The difference between the products resulting from the same principles, must be dependent on a specifically different action, promoting different affinities, which must also be influenced by its primary constituents — The above principles are corroborated by the phenomena of Inoculation — If the bud of one tree be engrafted into the branch of another, it continues to flourish retaining its own specific properties, tho nourished by the same sap which imparts very different properties to the tree with which it is inoculated —

The same principles are applicable to the inferior orders of animals. If Polypi be fed on fluids of any particular colour, as red, black &c. they will acquire the colour of their nutriment. See Adam's Micrologia —

* This was a long tube communicating with the external air through which he breathed

Having now endeavoured to prove, first that the absorbents commence on the exterior surface, most probably of all animate bodies; and stated certain facts founded on experiment, proving that in certain instances fluids are taken up from the surface - Secondly endeavoured to point out the mode by which this effect is produced, the great variety of new modifications of matter consequent on it, with a suggestion founded on the principles of absorption, the assimilating powers of the animate creation, and number of elementary articles already detected composing the vital machine; that all matter in nature is capable of receiving such modification, as to become alimentary to some links in the chain of animal matter -

I shall now take a view of a few of the experiments which have been instituted, denying cuticular absorption, and endeavour to shew wherein they are insufficient to fulfill their intention -

The following experiment instituted by Dr. Dangerfield, at first view appears to carry considerable weight — "On a fine morning in april (says the Doctor) the temperature of the room in which I proposed to make the experiment being between 70 & 80 degrees of Fahrenheit, I adjusted my tube as formerly directed,* took off my clothes, placed the end of the tube in my mouth, stopped my nose, and directed my assistant to besmear my body and superiour extremities by means of a sponge, with Spirits of Turpentine The pleasant sensations produced by the friction of the sponge,

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"counterbalancing the painful ones created by the turpentine, induced"
"me to direct the attendant to continue it without intermission. The ap-"
"plication of this fluid, therefore, so far from having been intermitted,"
"was unceasingly continued throughout the experiment, so that the absorb-"
"ents had the additional advantage of friction, by which to drink up"
"the substance with which their mouths were besmeared — Three quarters"
"of an hour elapsed, when my assistant received my urine in a vessel as"
"I sat, but could not discover the least smell of Violets. The hour being"
"completed, my body was carefully washed — I then closed my mouth"
"and nose, and walking precipitately into the next room, examined my"
"urine without being able to detect in it the odour by which the ab-"
"sorption of turpentine was to have been proved. My friends also ex-"
"amined my breath, and were unanimous in saying it had undergone"
"no change. The common tests were frequently resorted to during the"
"succeeding 24 hours, but with the same results — —"

To the above experiment, though at first view so plausible, the following objections arise. The period at which the urine was taken was too soon after the commencement of the experiment, an objection substantiated by an experiment hereafter to be noticed. The examination of the urine was made by his assistant, whose olfactories being immediately exposed to the fumes of the turpentine during the experiment, must have been very much obtunded. After the expiration of the hour, the Doctor observes that he himself, having first been washed, retired into the next room, and examined his

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urine. Here the objection made to the assistant, applies with equal force, admitting that the turpentine was absorbed. As the Doctor has not informed us with what he was washed, an objection still more serious may arise, as the odour of the turpentine cannot be washed from the skin by cold water, nor yet by warm water and soap but with the utmost difficulty – It is therefore presumable that the odour of the turpentine, whilst the Dr was examining his urine, so stimulated his olfactories, as to render them completely unsusceptible of impressions from the more feeble odour of violets, even though it did exist ever so perfect. This suggestion is strengthened by the circumstance that examination was made in the next room, which from the expression we infer was an adjoining room, the intermediate door was probably open during a part of the experiment, and must have been so when he retired His sense of smell, therefore, (admitting the turpentine all to have been washed off his skin) must have been assailed by the odour which escaped from the room in which the experiment was instituted, into that in which examination was made – Examination of his breath was made by his friends, he does not mention at what time, nor where, Circumstances, of primary importance – I therefore consider the experiment unconclusive, first because the term of its continuance was not sufficient – Secondly. Examination of his urine was made by persons not calculated to determine the point. Thirdly. The time of

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examination of the breath, and where it were made, are not specified; nor whether the persons examining it were previously exposed to the odour of turpentine; and is therefore equivocal.

Several ingenious experiments have been instituted by Dr. Clapp relative to the present question, but from the smallness of the surface exposed to the different agents which he used for the purpose, no conclusive inferences on the subject could be drawn. From the introduction of his arm through mercury at a low temperature together with its gravity; there is every reason to suppose the absorbent vessels of this surface, were enervated, and rendered incapable of taking up much, and the little imbibed could not be expected to impart any of its specific qualities to the urine or breath.

Dr. Curry has instituted some experiments on the warm bath, which merit attention. The Doctor found that after bathing in the Buxton bath, the temperature of which was 82° of Fahrenheit, the weight of his body was not increased, but if any thing, diminished. He also found that in a case of Dysphagia, where a bath at the temperature of 96° was used; no increase of weight took place, though the patient after being carefully dried, immediately stepped on a balance which would have detected a single dram. From the above experiments we find that no increase in the weight of the body takes place during bathing — According to the experiments of Mr. Cruikshank, heap-

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pears to have perspired upwards of seven pounds in twenty four hours at the temperature of 71°. If then at the temperature of 71°, seven pounds were perspired, certainly it is reasonable to infer that a much larger quantity is perspired at the temperature of 82° in the same period of time. At the temperature of 96° it must have still been proportionably increased. In the latter case the Doctor conceives, that did cuticular absorption at all take place, it would have done so in the present case. The plastic powers of nature, always submissive to her calls, would in the present instance have supplied her wants. Though the Doctors observations be correct, they do not at all disprove the doctrine of Cuticular absorption. The temperature of 96° acting as a gentle stimulus on the body in its then weak and emaciated state, must have induced perspiration, daily observation proving that effect to be more readily induced under those circumstances, than in health.

From the above facts the following conclusions are deducible. The same circumstances influence the action of the exhalant & absorbent vessels. Their regular action on the surface as well as in the more internal parts of the body, is necessary to keep up the equilibrium of the system. If therefore the absorption be equivalent to the exhalation, the weight ought to be precisely the same, but if

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the exhalants on the surface, as their brothers in the more internal parts of the body, throw out more than the absorbents take up, a diminution of weight ought to take place, admitting the lungs to support their equilibrium (the contrary of which I believe has not been proved) that the exhalant vessels or capillary arteries, in the system, do in a healthy state throw out more than is taken up by the absorbents, is proved by the phenomena of growth. From the above we learn, that nature ever fond of order, does not in this instance break through what appears to be a general law; that all excretory surfaces are also absorbent surfaces, as all the membranes of the human body & also the lungs —

Taking it for granted that absorbents do originate from every part of the body interior to the cuticle, I instituted the following experiment with a view to ascertain whether the absorbents would take up garlic in sufficient quantity, to render its presence perceptible in the urine —

J. M. aged 60 years, having an ulcer on his leg about 8 inches in circumference, his nostrils filled with lint and confined with adhesive plaster, breathed the external air through a tube; an ounce of the expressed juice of garlic with an equal quantity of milk were confined to the surface of the ulcer, by means of half

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a bladder cut transversely, the cut edges being made to lay flat by means of small fissures, were attached to the sound skin by means of adhesive plaister, over which bandages were rolled to retain it in its situation; the fundus of the bladder having a pipe adapted to it through which the fluid was poured, and the pipe corked. In this situation he continued to breath through the tube Fifty minutes, when it having become considerably obstructed with perspirable matter, he began to respire with difficulty - A quantity of his urine was now obtained in a vial, corked and conveyed out of the room, when it was very particularly examined by persons not exposed to the odour of garlic, yet the slightest smell of it could not be distinguished - Leaving the tube and restraining his respiration, he precipitately retired into the next room; on examining the leg I found that none of the garlic had leaked out, and its smell being very small, he retired to the surgical ward, in which he was a patient - I desired a number of the patients to smell the bladder & inform me what were its contents, which they did, and some in a doubtful manner alledged that it was garlic. Finding the odour so exceedingly small, I concluded to leave it be on all night, and desired the man to retain his urine till the morning; when on the most minute examination, both by myself and a number of others, the slightest odour of garlic could not be detected. Not satisfied with the result of this experiment, as there could be no doubt of the existence of absorbents in

the parts, and in contact with the mouths of which the garlic must have come, I resolved to repeat the experiment.

Experiment 2nd. J. B. aged 57 years, having an ulcer on each leg occasioned by a scald; his nostrils were confined and a tube adapted as above, except that a shorter one, and that placed in an oblique position, to afford the perspirable matter an opportunity of passing off, was used. Bladders sufficiently large to encircle the ulcers, which were upwards of eighteen inches in circumference, were prepared and applied as above. – Half an ounce of the expressed juice of garlic with an equal quantity of milk, were introduced into each bladder & the pipe corked, he continued to breath through the tube for four hours, when the apparatus were removed from his legs, and they carefully washed, he now retired into the next room restraining his respiration, when he voided his urine, which was carefully examined with an expectation of detecting the odour of garlic, which was however disappointed. – His urine was afterwards repeatedly examined, but with the same result. –

To the above experiments it may be objected that the ulcers were old and chronic, and consequently the action of the absorbents weak: But certainly they go to shew us the absurdity of concluding that absorption does not take place through the cuticle, barely because a few inches thereof were exposed to an odorous body for a short time. – In both the above instances the expressed juice of garlic was brought in

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absolute contact with the mouths of the absorbent vessels, and yet they did not take it up in sufficient quantity to render it detectable - No person will certainly allledge that absorbents do not exist in an ulcer, the contrary may be proved to demonstration - A portion of the edge of an ulcer shall cicatrize in a short time, then in consequence of exercise, fever &c. blood shall be transfused under the cuticle, but by removing the causes of disease, and making a little pressure, shall again be absorbed in a very short time -

Though it is a fact notorious to all farmers, that the milk and Butter of Cows feeding on garlic, are strongly impregnated with its odour and taste; yet conceiving it possible that the odour of the garlic might reach the milk through the medium of the Lungs, I instituted the following experiment -

I obtained a bitch (a female was preferred on account of the greater facility of drawing off the urine by means of a Catheter) and having confined her, made an opening into the Trachea, into which I introduced a pipe communicating with the external air and which was confined in the Trachea by means of a ligature - The bitch now breathed exclusively through the pipe, as was proved by holding a polished Scalpel before her mouth and nose, it not displaying the smallest appearance of moisture - A ligature was now

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passed loosely round the esophagus, and a tube introduced into it nearly to the Cardia, when about ℥j of the expressed juice of garlic with an equal proportion of water, were introduced into the pipe by means of a tunnel, through which it passed into the stomach; immediately on the fluids reaching the stomach, the pipe was withdrawn & the ligature tightened lest it should be ejected by vomiting, which had taken place in a former experiment. She continued to breath regularly through the tube for four hours, when the ligature by which the urethra was confined was taken off, she immediately voided a quantity of urine, which was received in a vial, removed from the room, and examined; but so strong was the odour, that it smelled more like the expressed juice of garlic than urine, as was acknowledged by several persons who smelled it —

Being perfectly satisfied by the above experiment that garlic was taken up by the absorbents; and also that their assimilating powers did not destroy its odour, I had a quantity of the expressed juice prepared, and having a tube passed through a wooden pane placed for the purpose in my window, I commenced breathing through the tube; first having my nostrils filled with pledgets of lint and confined with adhesive Plaster; so that though I made several efforts to breath through them, I could not procure the least air. The expressed juice of garlic was now brought into the room, and long rollers dipped in it and made perfectly wet, were applied from my ancles to my necke; and occasionally moistened afresh with the juice by sponging — After some time, finding the garlic

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very irritating, I directed equal parts of milk somewhat warmed to be used — After breathing through the tube two hours, the bandages were taken off; and my skin carefully washed with warm soap-suds; Nevertheless some odour of the garlic remained upon my skin — I now restrained my respiration, and retired to another room, when some of my friends who had not been exposed to the odour of garlic, examined my breath, and could very distinctly observe the smell of garlic. The utmost attention was paid to distinguish the odour of garlic in my breath from that on my skin; but so strong was the former that they were unanimous in declaring no doubt existed with respect to it — My urine which was made near the conclusion of the experiment was corked up, conveyed down stairs & carefully examined, when the odour of garlic was so distinctly perceived, as to preclude a doubt of it's existence there — The urine voided some time after the conclusion of the experiment, was also examined, and with the same result. Though there could remain no doubt that the garlic had entered the system and displayed itself in the urine and breath; nevertheless, the mode of it's introduction was not so certain — A considerable time after the commencement of the experiment, I was sensible of the odour of garlic — Where it arose from I could not determine — I also experienced a degree of irritation in my Tracheae, and a distinct perception of garlic in my fauces: Yet as the adhesive plaster was not so completely close as at the commencement of the experiment; I conceived it possible

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that a little of the air of the room might possibly have passed through my nostrils, or that during inspiration some air might have entered my mouth exterior to the tube — Unable to determine whether the odour had entered my system through the lungs, or had been taken up by the absorbent vessels on the surface, conveyed to the blood, and made its exit at the lungs — This uncertainty induced me to repeat the experiment in the following manner, —

Experiment V.

A piece of pasteboard was adapted to the size of a pane of glass, in the middle of it a hole was made exactly suited to my neck, making a slight degree of pressure all round, a fissure was made from the edge into the hole, then by bending it was placed round my neck, adjusted in the window instead of a pane of glass, and completely confined by putty, as also the fissure — Sticking plaster was now put around my neck in such way, as to completely close any interstice which might exist between it and the pasteboard — All communication of air being now cut off as completely as a room well can be, and the small quantity which did circulate, passing in with rapidity; The temperature of the external air being 43°, that of the room 61° at the commencement of the experiment. Rollers soaked in equal parts of Milk and the expressed juice of garlic, were now regularly applied, commencing at the superior extremities, and continued over the body till they reached the knees — Bruised garlic in substance was placed in the axillæ and groins — At the commencement of the ex-

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riment my pulse beat 92 strokes in a minute, its natural standard. In 35 minutes it continued at 92, the temperature of the room had now risen to 68° — In 50 minutes, pulse 96 and somewhat irregular, thermometer 70 — I was sensible of a degree of irritation in my trachea though unable to distinguish the least odour of garlic; which that I might be convinced none had passed through the window, I had occasionally endeavoured to do from the commencement of the experiment. I now made some urine in a vial, which was immediately corked up and numbered. In 55 minutes I was sponged with milk and expressed juice of garlic equal parts; raised to the temperature of my body. In 65 minutes pulse 88, small and without tension, thermometer 71 — I felt convinced that I could perceive the odour of garlic when I expired, but though I used every endeavour to discover it when I inspired I could not. In 75 minutes, pulse 97, small and feeble, thermometer 72, I again voided some urine in a vial which was corked and numbered as before — I was fully satisfied from the sensations I experienced in my trachea and from the odour and taste of the garlic, which I distinctly perceived, that on examination it would be detected by others, and therefore spoke of concluding the experiment; but was advised to continue it till two hours should be elapsed — In 90 minutes, pulse 85, thermometer 73, the sensation of garlic in the trachea more acrid, & on making a full expiration, stronger, yet on inspiring I was not able to detect the odour. The air was somewhat brisk, so

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that the matter expired was completely dissipated before I could inspire – 105 minutes, pulse 82 very irregular and small, thermometer 75, I now passed urine a third time, which was managed as above – In 120 minutes, pulse 84, a little fuller and more tense than before, the experiment was considered as terminated, the rollers taken off and my body well washed with warm water and soap, and a blanket placed round me; when forcibly drawing the pasteboard out of the window, and restraining my breath, I precipitately retired down stairs to the shop, when my breath was accurately examined by my friend & Colleague Mr. Smith and several other Gentlemen, who unanimously concurred in declaring that they smelled the odour of garlic very distinctly on it, My Colleague Mr. Harris, who assisted me in the performance of the experiment, and who breathed the fumes of the garlic all the time, had his breath examined by the same gentlemen; who agreed that they could not distinguish the least odour of garlic on it – Not contented with their assertions, as they were acquainted with the experiment, and therefore their imaginations might be supposed to exert an influence, Mr. Harris and myself went into one of the wards, where they were entirely ignorant of the experiment; and out of five persons who smelled our breaths, four declared they could not distinguish any smell whatever on his, whilst on mine that of garlic was very perceptible – The fifth conceived that a slight smell was observable on his, yet not to be compared with mine in strength.

After the conclusion of the experiment, I made some more

* With a view to render my situation more comfortable, and consequently the experiment more decisive; two sheets and the same number of Blankets were placed round me, and removed only when I was sponged

urine in a vial as above; and Mr. Harris previous to quitting the room in which the experiment was made, also voided some in a vial, which was corked and marked in a different manner - There were now four vials which contained my urine; and Mr. Harris's, which were examined by the same gentlemen, not knowing one from the other - They were unanimous in declaring that No 1 and Mr. Harris's had not a perceptible smell of garlic - No 2 had a slight smell, but in Nos 3 & 4 it was obviously distinguishable, as will more fully appear from the following relation - In company with a friend, I took all the above vials into the Mens Surgical Ward, and without giving them the most distant Idea of their contents, asked each person, what they smelled - They almost to a man declared that Nos 3 & 4 had a very perceptible smell of garlic, No 2 had it in a slight degree; but that No 1 & Mr. Harris's had no odour but that of urine* -

This experiment was so conducted as almost to preclude the possibility of error - I neither had an opportunity of smelling the garlic while preparing, nor yet while the rollers were applying. My cuticle was perfectly sound; and least it might be alledged, that the odour might have penetrated through the more thin cuticle on the Glans Penis, or passed up the Urethra or Rectum; the former was prevented by confining the prepuce exterior to the Glans, the latter by being covered with adhesive Plaister - If as I conceive, no error took place in this experiment, it must be considered as conclusive.

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With a view to repeat some of Doctor Clapp's experiments, and at the same time obviate one or two objections which appeared to have considerable weight, the following experiment was instituted.

Experiment VI.

A large glass jar was inverted over water, and a bottle containing turpentine was then placed under the water and opposite the mouth of the jar, when the cork was drawn; the turpentine rose to the top of the water in the inverted vessel. This was done in the morning, and the experiment not commenced till the evening so that if any smell of the turpentine should have escaped from the Vessel, when brought into the room it's odour might be dissipated previous to the commencement of the experiment, before commencing which the temperature of the water was increased to 85 degrees. I then passed my hand and arm into the inverted jar, the turpentine surrounding my hand and half my fore arm. In about 25 minutes I felt very considerable irritation from the turpentine, which continued to increase till an hour and a half were elapsed, when feeling no symptoms of the turpentine in my system whatever, and the smarting of my hand and arm being very severe, I was about to terminate the experiment; but as several gentlemen were at this time engaged feeling my pulse with respect to which they differed somewhat, I postponed it a quarter of an hour longer, when feeling something acrid and irritating in my trachea which I

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could attribute to no other circumstance than the absorption of the turpentine; I resolved to continue the experiment - My friends now examined my breath, and all concurred in declaring that they smelled something peculiar in it, and different from what it was at the commencement of the experiment, for I then had it examined not knowing what odour the turpentine might acquire did it reach the lungs - Out of six gentlemen who were present, four declared decidedly, that they could perceive the odour of turpentine on my breath, and the other two, though they conceived they could discern a difference, were not so decided - I took a portion of urine in a vial previous to commencing the experiment, at the end of one hour I again made a little, at the end of an hour and three quarters, when I began to feel the irritation in the trachea mentioned above, I voided a little more, and again at the close of the experiment; which continued 3 hours - All the vials immediately on the urine's being taken, were corked and numbered, so that no mistake could arise. On the following morning I retained a portion of my urine - There were now five vials, all of which I took to the surgical ward, where they were examined. They declared that No. 1, the vial that was taken previous to commencing the experiment, and No. 5, taken the succeeding morning, had no smell different from urine. No. 2 they alledged had a smell

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somewhat different from the former. Nos 3 and 4 they declared had a smell different from urine; one alledged that it contained some vegetable, another that it resembled tea, and a third that it was like briar root —

On the afternoon of this day, no odour different from natural existing either in my urine or breath, I took twenty drops of spirits of turpentine, taking care previous to taking it to retain some of my urine in a vial, in an hour and a half after taking it, I again voided it, and in 3 hours some more; these were first examined by my colleagues and compared with what I had made during my former experiment. They were of opinion that if the smell of either could be compared to violets, Nos 3 & 4 of the former, and Nos 2 & 3 of the latter were best entitled to it. I then took them to the surgical ward as before. The first person who smelled them compared No 2 and 3 of this experiment to Nos 3 & 4 of the former, and declared that No 1 did not smell of any thing but urine, several others on smelling it, not being acquainted with his decisions, declared the same. I now obtained some violets and asked whether there was any alalogy between their odours, they conceived that No 3 & 4 in experiment No 6 and Nos 2 & 3 in experiment 7th did smell very considerably like it. For my own part, though I conceived I could distinguish a difference between the urine No 3 & 4 Expt. 6. & Nos 2 & 3 Expt. 7. and that which I voided under ordinary circumstances, I cannot

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declare that I could satisfactorily distinguish a great likeness in it to the odour of violets - Conceiving that I probably had not taken a dose sufficiently large to bestow the smell of violets to the urine, a friend who had been in the habit of taking it, condescended to oblige me.

Experiment VIII

Captain W— took upwards of ℥ij by measure of the spirit of turpentine; previous to taking it I procured a vial of his urine, in an hour and a half afterwards another vial of it was taken, and the following morning another portion was preserved. The turpentine was taken at six o'Clock in the evening. These three vials were exposed to the same examination as those mentioned above. The two last taken after the administration of the turpentine, were conceived to bear a strong analogy to those in the two last experiments, which there was reason to allege were influenced by the turpentine - Though the smell of violets was even in this instance so extremely indistinct, that probably not one person in twenty would have declared that it at all possessed it, provided they were not prepared to expect it.

Having now endeavoured to furnish my mite towards the progress of science; it now remains for me, Illustrious Professors, to bid you adieu. Not the brilliant scenes which youthful fancy paints can drown the regret produced by the reflection that I

am about to quit that sphere, so often illiminated by your supe-
rior talents and graced by your virtues — Accept, Gentlemen,
my unfeigned thanks for the many benefits you have conferred
upon me; and that health and long life may enable you to
bestow the same advantages on others, is my sincere wish.

FINIS.